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Production Economics: Worthwhile Investment?

C. Richard Shumway

DEPARTMENT OF AGRICULTURAL ECONOMICS

TEXAS A&M UNIVERSITY

COLLEGE STATION, TEXAS

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PRODUCTION ECONOMICS: WORTHWHILE INVESTMENT?

C. Richard Shumway

Abstract

Agricultural production economics research is examined within a broad framework of scientific development and utilization. Recent findings in three selected areas of the subdiscipline are examined and opportunities for further fruitful inquiry are identified. Few "stylized facts" emerge from recent work and suggest the need for much careful hypothesis testing, model exploration, and empirical sensitivity analysis in the future.

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PRODUCTION ECONOMICS: WORTHWHILE INVESTMENT?

"The secret of science is to ask the right question, and it is the choice of problem more than anything else that makes the man of genius in the scientific world."

-Sir Henry Tizard

Research investment in the field of agricultural production economics has been extensive. In fact, the agricultural sector may well be the most analyzed sector of the economy and agricultural production the most analyzed part of this sector. With so many other important areas of inquiry not yet pursued to the same extent, one may legitimately ask whether a continuation of relatively heavy investment in the subdiscipline of agricultural production economics is warranted.¹

Providing a reasoned response to such a question requires that we have a basis for judging among alternative research areas. Of all scientists, economists ought to have some clues about how to make such judgments. Certainly the marginal principle of economics should be relevant, at least as a management criterion. We simply examine the production functions for discovery and utilization of new knowledge, consider the prices of inputs and outputs, and equate marginal cost with marginal revenue (or the marginal rate of product transformation with the ratio of output prices when input levels are fixed). It is a simple and powerful economic concept; but, when applied to nonrepetitive production processes for which output occurs in very lumpy increments and for which output prices are unknown (like scientific discovery), it is not even modestly useful. The fundamental problem is not with the marginal principle but with the extremely large confidence intervals associated with nearly every piece of data needed for a relevant analysis. Only current input prices are reasonably certain.

Partially because of these large confidence intervals, much problem selection by individual scientists is driven more by curiosity than by a priori assessments of potential knowledge "production." This effect gives to some rationally-minded production types (like taxpayers) the impressions of research being conducted in ivory tower withdrawal from the real world and of an elite scientist class of people responsible only to their peers and protected from the competitive performance pressures of the private sector.

Since the marginal principle isn't helpful, perhaps the best that can be done pragmatically is to establish a general framework for assessing what economists are about, examine what has been learned from the subdiscipline of agricultural production economics, and consider what might be learned in the future. That is the approach I will take here. I will review in very broad terms what I think is the general purpose of the discipline, examine what has and has not been learned in a few selected (and only lightly reviewed) areas of recent production economics research, and suggest some remaining opportunities for agricultural production economics research.²

Purpose of the Discipline

Agricultural economists are clearly both creators and users of science. A particularly informative model of scientific creation is found in a book by Walter L. Wallace (1971): *The Logic of Science in Sociology* (see Figure 1). He suggests five informational components in the development of a science -- theories, hypotheses, observations, empirical generalizations, and decisions to reject or not reject hypotheses. He arranges these components in a circular diagrammatic model to suggest that there is no

inherent beginning or end. He does place the theory component at the top, but this placement is arbitrary and he emphasizes that many theories have developed only because of careful initial observation. However, the direction of movement between various subsets of the components is unambiguous and is determined by procedures which act upon the components. For example, logical deduction is used to derive testable hypotheses from theories; interpretation, instrumentation, scaling, and sampling to obtain observations for judging the hypotheses; measurement, sample summarization, and parameter estimation to draw empirical generalizations from observations; concept and proposition formation and arrangement to organize theories from empirical generalizations; formal tests of hypotheses to make decisions to reject or not reject hypotheses; and logical inference to develop or revise theories based on test results.

As agricultural economists we collectively seek to develop an economic science that will be more useful for the solution of real world problems in the future than the science is now. Nevertheless, there is no requirement in the Wallace model that the same scientist contribute to all components of the scientific process. Although few new theories come out of agricultural economics departments, many hypotheses are formulated and much data are collected to test hypotheses that help determine whether or not existing theories have empirical credibility. Each of us can make a contribution to the development of science even though we do not cover all parts of the scientific process. This process requires that our contributions fit in with components of other general and agricultural economists that necessarily precede or follow ours.³

As we examine the current scientific status of economics, we must

acknowledge that economics is a very soft science. We have well developed theories (at least logically organized), and we use quantitative techniques that give our discipline an appearance of rigor. These two qualities cause many scientific philosophers to rank economics as the most developed of the social sciences (e.g., Popper 1961, Myrdal 1973). Nevertheless, our applicability theorems are very loose and predictions based on our theories are often poor. There are several obvious reasons why this is so, but they generally boil down to the fact that there are simply no real constants in an economic system like there are in most natural sciences (Myrdal 1973). We don't operate in a closed system, so, when a prediction turns out to be false, we don't know with certainty whether one of the hypothesized general laws has been disconfirmed or whether the ceteris paribus conditions have not been fulfilled. This situation makes it extremely difficult to disconfirm a theory and results in the profession's steadfastly refusing to give up a well-developed simple theory even after there is considerable evidence that it does not relate well to empirical observations.⁴

Regardless of the soft scientific nature of our discipline, we must help make it more useful. We can't do a better job of serving relevant audiences in the future without current development of the science. The pertinent question thus becomes, "How likely is that to happen through agricultural production economics research?" Because of data availability and quality, agricultural production has long been a focal point for applying and testing micro and macro economic theories. Yet we have few "stylized facts" in agricultural production economics (or in any of our other principal subdisciplines). A stylized fact may be defined as an

empirical hypothesis that has been tested rather thoroughly and has not been disconfirmed. Nearly all important production hypotheses have been rejected by one test or another. Nevertheless, because of the lack of real constants in our systems, hypotheses must be carefully tested in many settings and from many perspectives before we can conclude that the theory doesn't adequately describe the real world. Significant contribution to the development of the science may require going full cycle through the Wallace diagram many times.

In addition to seeking to contribute to the development of science, we also strive to use the best economic and related sciences available to help solve current private and social problems. Important synergism is possible as dual roles are performed in developing and using science. The same procedures can often be used to test theories and also to help users of the theories be more effective managers. For example, observations must be collected, parameters estimated, and generalizations drawn both to test hypotheses and, when the hypotheses are maintained, to give relevant guidance to decision makers.

In judging the potential benefits from more research in a subdiscipline, it is particularly important to consider the extent to which the topic stimulates the imagination and creative resources of the individual scientist. Creativity is personal property and is not easily managed by external efforts. Without good ideas, we are as Jean-Henri Fabre's famous processionary caterpillars who spent a full week walking in circles around the top of a flower pot. We can mean well but make little or no real progress. Some excellent ideas on creativity have been provided by one of our own, George Ladd (1979, 1987).

One of the most deadly errors researchers can make is thinking that all the major (or at least the easy) discoveries have already been made. The consequence is to lower our expectations for our own work by assuming it can only contribute at the fringes, that it can only massage the fundamental contributions made by others.

To dispel such notions, one need only consider the state of physics at the turn of the century. By 1900, physicists had determined that the classical theory of physics rendered a complete understanding of radiation with the exception of two phenomena -- the photoelectric effect and blackbody radiation. The work of Planck in the early part of this century provided a solution to blackbody radiation, and, using similar reasoning, Einstein explained the photoelectric effect. In making these two discoveries, however, Planck and Einstein revealed important inadequacies in the classical theory. Their discoveries opened up the new field of quantum mechanics which, along with the general theory of relativity (introduced by Einstein in 1916), have replaced the classical theory as the fundamental foundation of contemporary physical thinking (Hawking 1988).

To see the translation of these and subsequent discoveries into implementable technology, one could profitably spend a few hours in Disney's Epcot Center or peruse Stephen Hawking's (1988) *A Brief History of Time*, a lay treatise on discoveries in physics. If the great minds of this century could create such a technological revolution and if physicists and astronomers could make such remarkable discoveries concerning the universe with no more ability to conduct controlled experiments than economists have, it is not inconceivable that our discipline can also make some phenomenal contributions to the future well-being of society.

Selected Findings from Agricultural Production Economics

I will turn now to a discussion of a few selected areas of recent production economics research, what I think has been discovered, and what remains unclear or inadequately dealt with. In doing so, I am not going to even pretend to be comprehensive in either areas covered or depth of coverage in any area. Some very important literature may be entirely overlooked or inadequately treated, for which I apologize in advance. My only defenses are limited time and ignorance. There has been no attempt to bias the conclusions or misrepresent the preponderance of evidence by deliberately failing to consider some studies.

I will consider only three areas -- behavioral objective, analytic simplification, and sensitivity to specification errors. These three areas were chosen because of their importance in model specification and because they can profoundly impact the value of the guidance we give to both public and private decision makers.

Behavioral Objective

The classical theory of the firm presupposes that producers seek to maximize (expected) profits. Fundamental micro and macro implications of the theory of the firm have been shown to rely critically on this behavioral assumption. Changing the underlying motivation can change producer behavior and industry performance, often drastically.

Because of the extreme uncertainty facing agricultural yields and prices, considerable attention has been given in recent years to determining farmers' goals and motivations. Much of the work has focused on the alternative hypothesis of utility maximization where utility includes

arguments of both profit and risk. Some of the work has also addressed leisure and consumptive objectives as well as hierarchical goals. Research has included both firm-level and aggregate studies. Findings have been mixed.

For example, consider the results from the 17 studies reported in Table 1. Each of three studies based on firm and individual respondent data found that the amount of risk faced was an important consideration in decision making. Considerable evidence of risk-averse behavior was found. The one firm-level study that did not formally address risk found that a hierarchy of goals was considered in the behavioral objective of farmers. Four of the 13 studies using aggregated data also reported important roles of risk variables in determining commodity supplies. And, one study rejected the hypothesis of profit maximization in favor of expenditure-constrained profit maximization for explaining aggregate behavior.

Conversely, although subject to a variety of problems in identifying truth by direct elicitation, nearly 90% of 377 surveyed Texas cow-calf producers stated that their primary goal was to maximize profits (Young 1989). In studies using aggregated data (for North Dakota, South Dakota, Iowa, Texas, Oklahoma, Arkansas, Louisiana, Mississippi, Georgia, the 10 USDA farm production regions, and Ontario Canada), the monotonicity and convexity properties implied by the profit maximization hypothesis were consistently not rejected by parametric test. The profit maximization hypothesis was also not rejected by stochastic nonparametric tests for each of the contiguous 48 states. Although individual nonparametric violations of the hypothesis were observed in all geographic units investigated, the extent of violation was minor. In all but one state, measurement errors of

3% or less would have been sufficient to give complete consistency of the observed data with the hypothesis of profit maximization. In very recent work, Lim obtained comparable results using data for the United States and each of the ten USDA farm production regions.

Further, Pope (1981), Estes, Blakeslee, and Mittelhammer (1981), and Taylor (1986) each cautioned that some of the evidence for significant risk-motivated behavior is questionable. Biased standard errors, stochastic random variables, and the nonlinear influence of stochastic variables on a risk-neutral behavioral objective were cited as reasons for apparent risk-motivated behavior when producers may have really sought to maximize expected profits. In addition, while violations of the closely related expected utility hypothesis of consumption theory have been found in experimental data, violations of all other tested behavioral hypotheses (e.g., regret, prospect, generalized expected utility, rank-dependent expected utility, and lottery-dependent expected utility theories) have also been found (Harless 1987; Starmer and Sugden 1987a, 1987b; Camerer 1989; Battalio, Kagel, and Jiranyakul 1990).

Despite the large amount of economic research that has gone into examining producer behavioral objectives, we still have not clearly established which types of farmers are or are not expected profit maximizers.⁵ Nor have we clearly established which farmers who are not profit maximizers nevertheless act as if they are. The hypothesis of profit maximization has been seriously challenged but not unambiguously ruled out as the "best simple theory" of motivation for firm-level decisions. Further, although not all empirical results agree, the

preponderance of evidence examined here suggests that the macro manifestations of agricultural firm decisions are not inconsistent with the implications of this hypothesis.

Analytic Simplification

Based on heuristic and formal hypothesis testing, considerable evidence exists to support some degree of analytic simplification in primal and dual modeling of agricultural production technologies. Among the important properties of the technology that justify analytic simplification are nonjointness, separability, homotheticity, constant returns to scale, Hicks-neutral technical change, and twice differentiability. The modeling implications of each of these properties will be reviewed briefly, and then recent evidence based on empirical tests will be presented.⁶

If outputs are nonjoint in inputs, the production, cost, profit, output supply and input demand functions for one output can be estimated without considering the impact of decisions made on other outputs. It is the hypothesis of nonjoint production that has been implicitly maintained so often in the vast literature in which production and supply relationships for individual agricultural commodities have been estimated without including alternative output quantities or prices as explanatory variables in the estimation equation.⁷

If outputs are produced by separable technologies, consistent aggregation and two-stage optimization is possible. That is, two models can be constructed (an aggregate model and an allocation model), each using a smaller dimension of exogenous variables, that collectively give exactly the same solution for an economic optimization as a single model with complete

output and input disaggregation. Separability of outputs and of output prices is the implicitly maintained hypothesis underlying the estimation of aggregate agricultural supply functions.⁸

A homothetic production function implies that expansion paths are linear out of the origin and requires estimation of fewer parameters to fully represent the technology. It is an implicitly maintained hypothesis in the extensive literature in which a Cobb-Douglas (or other homothetic) production, cost, profit, output supply, or input demand function has been estimated without first testing for functional form.

When production exhibits constant returns to scale, the average and marginal cost curves are horizontal and equal, and optimal output level is indeterminate for the competitive firm. In the aggregate, the elasticity of input price response to a change in output price is equal to the competitive industry's partial production elasticity for the input (Jorgenson and Fraumeni 1981).

When technical change is Hicks neutral, technological improvements do not affect the marginal rates of substitution of any pair of inputs or outputs.⁹ When time series data are used, Hicks neutrality reduces the number of independent parameters that must be estimated to fully reveal the technology.

If the technology is twice continuously differentiable, so is its corresponding dual model and the second derivatives of each function are invariant to the order of differentiation. Thus, price parameters in the system of output supply and input demand (or share) equations are symmetric.

None of these properties are implied by economic theory. They are all empirical hypotheses that may or may not apply to a particular production

system. With the obvious analytic simplification that is justified when one or more of these properties apply and the possibility of substantial error in inference occurring when they are assumed to apply but really do not, the need for careful testing is apparent. Several recent empirical tests for each of these properties are reported in the 21 studies noted in Table 2.

Short-run nonjoint-in-inputs production of all agricultural outputs was rejected using parametric tests for the U.S., Ontario Canada, six of ten USDA farm production regions, four of five South Central states, and Israeli farmers. The hypothesis of short-run nonjoint production of all agricultural outputs was not rejected for the remaining state, for the other four farm production regions, nor for Canada. Short-run nonjoint production among a variety of subsets of outputs was tested for four states; short-run nonjointness of all tested subsets was rejected in only one - Arkansas. However, among the other states, the subset of outputs exhibiting evidence of nonjoint production varied widely by state and data period. There is more evidence of short-run joint than nonjoint production in state, regional, and national data, and the empirical effect of binding allocatable inputs is generally stronger than that of technical interdependence when short-run jointness is evident in agricultural production.¹⁰

Agricultural output separability was rejected using parametric tests for the U.S. and for North and South Dakota. It was rejected using nonparametric tests for the U.S. and for 37 of the contiguous 48 states. Agricultural input separability was rejected using nonparametric tests for the U.S. and for 24 of 48 states. Separability of subsets of outputs and/or inputs was not rejected using parametric tests for the U.S., North and South

Dakota, five South Central states, or Westside California cotton production. Separability in various subsets of outputs and inputs also was not rejected using nonparametric tests for the U.S. or any of the 48 states. As with nonjointness, the nonrejected separable subsets vary widely among geographic units.

Homothetic agricultural production was quite consistently rejected using parametric tests. For example, of the nine studies reporting homotheticity tests in Table 2, homotheticity in outputs and/or variable inputs was not rejected for only two, Oklahoma outputs and Texas field crops and variable inputs. Like separability, homotheticity in subsets of outputs and/or inputs was not rejected in any area tested, and the homothetic subsets also varied considerably among states.

Constant returns to scale in agricultural production were rejected using parametric tests for Japan, Ontario Canada, and Indian farmers, but were not rejected using nonparametric tests for most of the 48 states. Parametric test results for the U.S. and Canada were mixed.

Except for gross-output Hicks neutrality for the U.S., Hicks-neutral technical change was rejected in all areas tested parametrically. It was not rejected using nonparametric tests for the U.S.

Tests of symmetric price parameters in the output supply and input demand (or share) equations (implied by a twice-continuously-differentiable technology) yielded mixed results. Symmetry was rejected for pre-World War II U.S., for Canada, and for Texas field crops. It was not rejected for Italy, post-World War II U.S., U.S. dairy production, or for Canadian or Japanese input demands.

So what are the stylized facts that emerge from these tests for analytical simplification? Consider four based on the preponderance of evidence:

1. Little evidence supports the hypothesis that technology is homothetic in all variables or that technical change is Hicks neutral. Therefore, neither the Cobb-Douglas nor the CES functional forms are suitable for modeling agricultural production or the associated dual specifications. In addition, other functional forms that maintain homogeneity, such as the homogeneous generalized quadratic mean, are not suitable choices for modeling agricultural production.

2. Production of some outputs is nonjoint in the short run. Thus, short-run production of some agricultural outputs can be modeled without regard for the decisions made on other outputs.

3. Some input subsets and some output subsets are both separable and homothetic. They can be consistently aggregated for multistage optimization using either primal or dual models. These properties justify analytical simplification by reducing the number of variables required in each model. However, for complete analysis of the disaggregated variables, multiple models must be constructed.

4. Nonjoint, separable, and homothetic subsets vary widely among observation units and model structures. Test results on constant returns to scale and symmetry are also mixed. These findings emphasize the need for widespread empirical testing of these properties prior to generally maintaining simplified analytic specifications and/or twice-differentiable production functions.

Sensitivity to Model Specification

Several researchers have documented much sensitivity of empirical results, with great practical importance for decision making, to choice of functional form (Swamy and Binswanger 1983, Saez 1983, Chalfant 1984, Baffes and Vasavada 1987, Howard and Shumway 1989, Fawson, Shumway, and Basmann 1990). The sensitivity is not limited to comparisons of functional forms which differ in number of independent parameters, such as first-order versus second-order Taylor series expansions. The sensitivity carries over to comparisons among alternative second-order expansions, even at the point of expansion.

For example, we recently re-estimated Ball's (1988) U.S. output supply and input demand model with the normalized quadratic and generalized Leontief functional forms. All the same properties maintained in Ball's translog model (i.e., constant returns to scale; homogeneity, symmetry, and convexity of the profit function in prices) were maintained in the estimation of our models. No other properties were imposed. The same data were used. Elasticities were computed at the point of approximation and compared with Ball's. All of Ball's translog own-price elasticities were larger than the corresponding normalized quadratic and generalized Leontief elasticities and some were several times larger. In addition, while the translog estimates suggested gross complementarity among all outputs and among all inputs, the normalized quadratic estimates did not reveal gross complementarity among all pairs of inputs. The generalized Leontief estimates revealed gross complementarity only among 2/3 of the outputs and 2/5 of the inputs.

Other specification issues that have been observed to substantially impact important practical results include observation unit, specification of fixed factors, indexing for data aggregates, maintained statistical hypotheses, and choice of static or dynamic model. Polson (1989) rejected the hypothesis of identical technologies among each pair of five South Central states. McIntosh (1987) found substantial differences among states in forecasting performance and nonnested test results for alternative specifications of expected output prices and commodity program variable definitions. Fawson, Shumway, and Basmann (1990) found sensitivity of hypothesis test results and elasticities to alternative error structures. Even asymptotically-equivalent estimation procedures can render substantially different empirical results (e.g., Shumway, Alexander, and Talpaz 1990). The well-known stylized fact that emerges from this area of inquiry is that empirical results are highly sensitive to model specification and estimation procedure.

Some Opportunities

While there is considerable evidence of risk-averse behavior among agricultural producers, it is not so apparent in most aggregate data. Perhaps the law of large numbers is responsible for diffusing the effects of risk responsiveness when data are aggregated. There is clearly a need to conduct more rigorous tests for behavioral objective of agricultural producers manifested in various data of concern. It may well be that the hypothesis of profit maximization can be maintained in aggregate agricultural analyses with little adverse impact on the reliability of estimated inferences relative to the inferences obtained from a more

accurate behavioral objective. However, maintaining this hypothesis without formal test is less likely to be a satisfactory practice in micro-level analyses. More attention to predictive performance and more powerful tests of the implications of alternative hypotheses is needed.

The evidence cited here provides little hope for simplifying agricultural production models because of overall homothetic or Hicks-neutral structures. Nonjointness, separability, and/or homotheticity in subsets are more likely to be legitimate justifications for analytic simplification, but many more tests will be required before any generalizable guidance can be provided. Because the nature and extent of simplification has varied so greatly with the unit of analysis, observation period, and model specification, some exploration of alternatives is currently warranted in designing specific empirical models.

Because of their potentially important impacts on economic inference, additional empirical testing and sensitivity analysis for constant returns to scale, twice-continuously-differentiable technology, and functional form should be conducted within the confines of specific empirical problems. These properties are not implications of economic theory but are frequently maintained hypotheses to facilitate econometric estimation. Because conclusions can differ based on type of production activity, observation unit, commodity aggregation level, and variable specification, a wide range of tests may be needed to guide model specification. Where empirical evidence is not very helpful in choosing from among alternatives, the sensitivity of results to a range of plausible alternatives should be examined and reported.

I have not addressed dynamic production behavior. Although of long standing concern to economists, this subject has been only lightly touched in empirical work. Static models remain the norm in production economics model design. The division of inputs into fixed and variable categories depends upon the production period assumed for adjustment, and, more often than not, the placement of inputs into one category or the other is at least partially arbitrary. Formal testing for fixed inputs could be conducted either by nested hypotheses within a dynamic model or by such means as exogeneity tests (Weaver 1977). Much more formal attention to dynamics is warranted. Even though dynamic econometric modeling has undergone important evolutionary development such that estimated rates of adjustment now often acknowledge costs of adjustment and depend on the extent of disequilibrium in each input, most models retain a structure in which the rate-of-adjustment matrix is a matrix of constants.¹¹ Optimal control and dynamic programming approaches to dynamic problems can surmount these restrictions but at a cost of increased complexity.

The profession has given increased attention in recent years to quality of data. The Economic Statistics Committee of the AAEA has focused the profession's attention on problems resulting from reductions in federal funding for data collection in agriculture. An AAEA Task Force issued a report in 1980 on problems in the USDA agricultural productivity data series. The Economic Research Service has recently adopted many of the proposals of the Task Force as implemented by Eldon Ball and incorporated them into revisions of the national productivity data series. Increasing attention to the quality of data used both for hypothesis testing and for inference remains a high priority for all concerned with the real world

applicability of our products.

As we seek to better use economic theory to guide public and private decision makers, several questions deserve greater attention than they have received in the past. What is the impact (including program cost, producer income, and consumer prices) of governmental intervention into one commodity on supplies both of program and nonprogram commodities?¹² What is the impact of emerging food safety, water quality, and related environmental legislation? What is the impact of changing international markets (especially those evolving because of major political change such as in Eastern Europe)? What is the distribution of benefits and costs from changing policies and international markets (a) among producers, input suppliers, value-added businesses, and consumers, (b) among income groups, and (c) among geographic areas? Since the primary concerns deal with the future impact of possible changes, what is the degree of uncertainty in the expected impacts?

Capitalizing on Opportunities

As might have been predicted from my continued interest in agricultural production issues, I am convinced that this area of research remains a relevant area for economic inquiry. A rich set of potentially fruitful issues remain to be addressed which could be supported by relatively high quality and abundant data.¹³

To be most fruitful both for the development of science and for using the science to solve societal and private problems, relatively more attention needs to be given to fundamental hypothesis testing of economic and statistical theory that permit simplified analytic models to be used.¹⁴

For an applied discipline such as agricultural economics, increased professional attention to technology transfer from basic to applied research is needed. We have an outstanding technology transfer infrastructure (the Extension Service) for getting results of applied research into implementable applications for agricultural producers. We do not have as well developed an infrastructure for transferring new discoveries and theories from the frontiers of basic research to those best equipped to do high quality applied research.¹⁵

Wallace's model of science does not require that the same person or even the same organizational entity do all the important types of work relevant to the development of the science, but it does require communication linkages from people working on each informational component. If we are going to help advance economic science so that it is more useful for addressing real world problems in the future than it is now, we must give more attention to communication linkages with those who work on the informational components that necessarily precede and follow ours. We must also attach more importance to efforts to advance the science at the same time we are using the science to provide guidance to current decision makers. Some of those efforts will be competitive, but many of them can be synergistic. Certainly over time they are entirely synergistic, and we could profitably take a longer view of the potential of our individual and collective efforts to help society.

How can we promote technology transfer from basic researchers to those of us who are mainly applied researchers? One way would be for applied researchers to assume the primary responsibility of this technology

transfer. Basic researchers have little incentive to do it just as applied researchers seem to have little incentive within our academic infrastructure and reward systems to engage in serious technology transfer of applied research results to implementable applications. Many of us are quite content to leave technology transfer of applied research results to extension specialists. If we are going to justify this behavior by the incentive structure, then with logical consistency we cannot criticize basic researchers for not helping more to facilitate transfer of their contributions to us. While we might all benefit if they would, the incentive structures favor our doing that. If we are going to do it, we need to spend some time in the basic literature and with the basic scientists in economics and other behavioral disciplines searching for theories and ideas that warrant our hypothesis testing and application to agricultural and related problems. This process could require our becoming better educated in economic and related theories and in quantitative methods and that we work to retain and improve those skills.

Our journals and journal reviewers can help by giving increased publication support for such technology transfer efforts. Those who are in the best position to communicate clearly the practical relevance of a new theoretical development to applied researchers may not be in the best position to actually apply the concepts in empirical research. Some who have an excellent grasp of theory may not be equally competent in quantitative methods or data management. Thus, some potentially relevant articles may lack empirical application when first presented in language that can be used by applied researchers. We may need to be willing not only to tolerate a few exceptions to our expectation of predominantly empirical

research but also to encourage and facilitate them.

Those who work in this basic-applied technology transfer arena need to be highly skilled in at least two economic languages, the one they primarily read (highly mathematical and esoteric) and the one they primarily write in (accessible to the general body of applied economists). Some argue that to do this task well, they also need to do some personal scholarly work that cuts across basic and applied research.

It will be a continuing challenge to strive to be serious contributors to the development of the science of our primary discipline, economics, and in using the best currently available science to provide useful information to existing decisionmakers. We will need to conduct serious and exhaustive tests of potentially relevant theories, revise or develop new ones, and use nonrejected theories for decision-making guidance. I don't know whether other areas of inquiry might be more fruitful during the next decade or two, but it is apparent that agricultural production economics is not likely to be fruitless.

Footnotes

1. Because of the necessarily narrow and incomplete nature of this paper in coverage of the topic, I have chosen to include in footnotes a number of the anonymous reviewers' comments that provide different perspectives. I have tried to refrain from editorializing on their views and have succeeded in all but two cases. Noting this past investment history, one reviewer noted:

"Amazing, then, is it not that there remain so many production problems... e.g., soil erosion; excess use of irrigation water, chemicals, fertilizers leading to water quality and environmental degradation; inefficient (too large and too industrialized) farm operations causing problems with the long term sustainability of agriculture; devastated agricultural communities, contributing in no small way to a vast array of social ills, etc. The focus of production economic research may have been misplaced...."

2. Since the entire domain of production economics may be overhauled at any time (as noted by another anonymous reviewer), such opportunity forecasts are inherently risky. They are also value laden based on the forecaster's own unique set of experiences, preferences, and notions. Thus, what follows clearly fits that cast.

3. The fitting together of various components of the scientific process could occur as a result of the individual's conscious effort to organize his/her personal place within the building block scheme of science. Alternatively, the market for ideas could accomplish this organization indirectly. It is entirely possible that Einstein didn't know Eddington or any other empirical physicist who could test the general theory of relativity when he first introduced it. Yet, the competition for ideas insured that others were there to test his remarkable idea. The competition for ideas is likely also sufficient now to insure that capable people are present to test and ultimately apply useful ideas.

4. Observed data are also sometimes very different from our economic concepts. For example, individual transaction prices can be observed if sufficient care is exercised, and various averages of them can be computed, but there is no observed price that relates unambiguously to the concept of "expected" price.

5. The first cited reviewer suggested that an even bigger problem exists:

"[There has been] little joint work with behavioral psychologists to really find out the character of the objective functions [and] little joint work with the social psychologists and sociologists and political scientists to identify the context for decision making.... What we should be doing is finding out what the real producers in the real world are all about rather than assuming and hypothesizing and testing, usually with rather flawed secondary data. What is more fascinating, really, is how the underlying institutions (laws, rules, regulations, customs, habits) reflect and influence real farmers and other firm managers, what kind of real behavior in real systems really occurs."

6. Analytic simplification is not the only motivation for conducting tests of these properties. Sometimes particular data don't permit detailed estimation, e.g., grouped data. Generalized separability test results from other data then could provide justification for the study of groups using data which lack detail. In addition, tests of technical properties facilitate the search for regularities in micro data that constrain macro behavior.

7. The recent agricultural economics literature has included several theoretical contributions to the development of nonjoint concepts [Shumway, Pope, and Nash (1984, 1988), Lynne (1988), Paris (1989), Chambers and Just (1989), Moschini (1989)].

8. Consistent aggregation of both quantities and prices into indices requires homothetic separability of the technology (Lau 1978). For an

excellent treatise on alternative primal and dual tests of sufficient conditions for consistent aggregation, see Pope and Hallam (1988).

9. When technical change is indirectly Hicks neutral, technological improvements do not affect input quantity ratios. Indirect Hicks neutrality implies Hicks neutrality if the production function is homothetic or if it is additive in time.

10. The first reviewer cited above argues that joint production is intuitively obvious in agriculture and that

"... the really important questions arising in the face of joint costs are not generally being addressed. The agricultural industry is foundering while very talented, creative agricultural economists worry about the trivial question of whether or not there is truly jointness while not addressing why it exists, or describing its character, or really trying to solve real problems arising therefrom."

In response, I would ask why, if joint production is intuitively obvious, so much agricultural economics research and extension work has been conducted assuming nonjointness (e.g., single-output supply functions estimated with a single exogenous output price and partial budgets that distribute in arbitrary ways the cost of inputs used to produce multiple outputs). The reviewer is correct that there are other important problems to be addressed, but it is not obvious a priori how seriously the assumption of nonjointness detracts from correct economic inference. That is an empirical question that may or may not shed light in the search for laws of economic behavior (see footnote 13).

11. The strong restrictions imposed on many dynamic model structures were sharply criticized by a second anonymous reviewer:

"[E]mpirical studies based on dynamic models have been wonderful examples of souls sold to the devil in charge of excessive abstraction and simplification for the sake of empirics."

The first reviewer suggested that "evolutionary economics" may lead to more productive research dealing with dynamic production behavior.

12. From the first reviewer:

"It seems we, the people in a representative democracy, should have some say in how all production should take place. Are not farmers (and professors) also a part of this democratic process? Why are you taking the ideological position that community is not important? In my view, that is the very problem with modern production economics. One need only go back to the old farm management economics to see how important community really used to be in economic analysis of production problems. Now we have this sterile, nonreal production economic analysis that has usefulness only to those who wish to push various ideological positions, namely to maximize economic growth, via encouraging the pursuit of an unhampered self-interest at the cost of a destroyed environment, destroyed agricultural communities, etc., all of which leads ultimately to a nonsustainable agricultural system."

13. A third anonymous reviewer challenged:

"[C]riteria such as usefulness, fruitfulness, potential benefits, etc. have never been the motivation and the engine for fundamental discoveries in science. Why should they be in the science of economics? The conclusion is that production (as well as consumer) economists should work for discovering the laws of economic behavior and not for being fruitful, relevant and useful to public and private decision makers. On the basis of this criterion, investment in economics, including production economics, is a foregone conclusion."

I agree fully with the reviewer's primary conclusion that we should work to discover "laws of economic behavior." Further, as we discover those laws, our work cannot help but be "fruitful, relevant, and useful to public and private decision makers," regardless of the social end sought.

14. Again from the first reviewer:

"What we need is a pluralism of methodologies and of theories brought to bear on real problems of real people in the real world. In simple terms, we need to move back toward farm management research like it was originally practiced, a true systems analysis that made the individual paramount, but only in the context of the community the individual was a part of, and within the context of the real physical and biological setting of real farms."

15. Agricultural experiment station researchers do some of this transfer. However, the emphasis of the experiment stations is on the development of farm technology and on doing applied research, not in communicating new basic research technology to applied research implementers.

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Table 1. Behavioral Objective Test Results

Source	Unit of Observation	Type of Data	Test Results
Lin, Dean, and Moore (1974)	6 California farms	Firm	Improved predictions with risk in utility function
Just (1974)	California field crops	District	Significant risk parameters in supply
Traill (1978)	U.S. onions	National	Improved predictions with risk in supply
Harper and Eastman (1980)	New Mexico small farmers	Firm	Goal hierarchies evident
Binswanger (1981)	330 Indian farmers	Experimental	Considerable risk-averse behavior
Weaver (1983)	North and South Dakota	State	Profit maximization not rejected ^a
Lee and Chambers (1986)	U.S.	National	Profit maximization rejected against expenditure-constrained profit maximization
Antle (1987)	Indian farmers	Firm	Risk-averse behavior evident
McIntosh (1987)	Texas and Iowa	State	Profit maximization not rejected ^a
Moschini (1988)	Ontario, Canada	Province	Profit maximization not rejected ^a
Shumway and Alexander (1988)	10 U.S. farm production regions	Regional	Profit maximization not rejected ^a
Aradhyula and Holt (1989)	U.S. broilers	National	Price variance important determinant of supply

Table 1. Continued

Source	Unit of Observation	Type of Data	Test Results
Polson (1989)	5 South Central States	State	Profit maximization not rejected ^a
Lim (1989)	48 U.S. states	State	Profit maximization not rejected by stochastic nonparametric test ^b
Maligaya and White (1989)	Georgia	State	Profit maximization not rejected ^a
Chavas and Holt (1990)	U.S. corn and soybeans	National	Risk important determinant of acreage allocation decision
Shumway, Alexander, and Talpaz (1990)	Texas field crops	State	Profit maximization not rejected ^a

^aEstimated output supply and input demand (or share) equations tested for consistency of the profit function with convexity and monotonicity in prices. These properties are implications of the assumption of price-taking profit-maximizing behavior of firms.

^bIn all but one state, measurement errors of 5% or less in the quantity data were sufficient for consistency with the hypothesis of profit maximization.

Table 2. Analytic Simplification Test Results

Source	Unit of Observation	Short-Run Nonjoint in Inputs	Separability	Homotheticity	Constant Returns to Scale	Hicks-Neutral Technical Change	Symmetry
Lau and Yotopoulos (1972)	Indian farms				R ^a		
Weaver (1977)	North and South Dakota		R outputs R crops R capital-petroleum R materials-petroleum F materials-fertilizer	R		R	
Brown (1978)	U.S.					R ^b	
Lopez (1980)	Canada			R	R		F
Ray (1982)	U.S.	R crops, livestock	R outputs, inputs		R		
Shumway (1983)	Texas field crops	R field crops R wheat and hay F wheat	R variable inputs R 4 crops F cotton-sorghum-corn	F variable inputs F outputs			R
Chalfant (1984)	U.S.			R			
Lopez (1984)	Canada	F crops, livestock					R ^b
Rossi (1984)	Italy						F

Table 2. Continued

Source	Unit of Observation	Short-Run Nonjoint in Inputs	Separability	Homotheticity	Constant Returns to Scale	Hicks-Neutral Technical Change	Symmetry
Antle (1984)	U.S.			R		R	F ^b
Grisley and Gitu (1985)	Mid-Atlantic turkeys			R			
Capalbo and Denny (1986)	U.S.		R partial materials-technical		F	F gross output R net output	
	Canada		R partial materials-technical		F	R net output	
Kuroda (1987)	Japan			R	R	R	F
Pope and Hallam (1988)	Westside CA cotton		F nitrogen-water				
Shumway and Alexander (1988)	10 U.S. farm production regions	R in six regions F in four regions				R in all regions	
Moschini (1988)	Ontario, Canada	R unrestricted outputs			R		
Chavas and Cox (1988)	U.S.		R ^C outputs R ^C inputs R ^C capital-labor F ^C capital F ^C labor F ^C materials			F ^C	

Table 2. Continued.

Source	Unit of Observation	Short-Run Nonjoint in Inputs	Separability	Homotheticity	Constant Returns to Scale	Hicks-Neutral Technical Change	Symmetry
Ball (1988)	U.S.	R	R outputs	R affine homotheticity			
Howard and Shumway (1988)	U.S. dairy						F
Lim (1989)	48 U.S. states		F ^c inputs in 24 states F ^c outputs in 11 states F ^c subsets of 3-5 inputs in 41 states F ^c subsets of 2-18 outputs in 44 states		F ^{cd} in 47 states		
Chambers and Just (1989)	Israeli farms	R ^e					
Polson and Shumway (1990)	5 South Central states	F outputs in LA F subsets of 1-5 outputs in TX, OK, MS R each output in AR	F fertilizer-misc. inputs in LA R each input pair in TX, OK, AR, MS F subsets of 2-5 outputs in TX, OK, AR, LA, MS	F subsets of 2 inputs in OK, LA, MS R each input pair in TX, AR F outputs in OK F subsets of 3-6 outputs in TX, AR, LA, MS			

^aF means the author(s) failed to reject the hypothesis; R means the hypothesis was rejected at the chosen level.

^bRejected at 5% level of significance but not at 1% level.

^cNonparametric test.

^dUsing criterion that probable measurement error in quantity data did not exceed 10 percent. Constant returns to scale would not have been rejected in 38 states with 5 percent measurement error as the criterion.

^eAlso rejected long-run nonjoint production in inputs.

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